

THE ANALYST.

JUNE, 1893.

PROCEEDINGS OF THE SOCIETY OF PUBLIC ANALYSTS.

THE usual Monthly Meeting of this Society was held on the 3rd May, at the rooms of the Chemical Society, Burlington House. In the absence of the President, Mr. Alfred H. Allen occupied the chair.

The minutes of the last meeting were read and confirmed.

The following gentlemen were proposed :—As Members, Mr. George H. Allibon, Belfast; Mr. Anthony Roche, Dublin; Dr. H. J. Alford, Taunton. As Associates, Mr. Chas. Harrison, Mr. G. E. Scott Smith, Mr. L. K. Boseley.

The following gentlemen were duly elected :—As Members of the Society, Mr. William James Orsman, Wigan; Mr. Charles E. Sohn, Anerley. As Associates, Mr. A. H. M. Muter; Mr. C. Hubert Pope.

The following papers were then read by Mr. Boseley :—

THE ACTION OF HEAT ON MILK.

By H. DROOP RICHMOND AND L. K. BOSELEY.

THE action of heat on milk has been studied chiefly regarding the changes in the albuminoids (c.f. Faber, ANALYST, xiv., 141). In a paper by one of us (ANALYST, xvii., 225) it is stated that the specific rotary power of milk sugar is reduced by heating.

The following figures obtained on milks heated to a temperature of 100° C. (boiling water) in closed vessels for different lengths of time will show the extent of reduction. The time of heating, unless stated, was about two hours, or, more exactly, it varied from 1½ to 3 hours. We had intended to heat milk for different times in order to see if we

could find a constant variation for a unit of time; but the results of Nos. 5 and 6 show conclusively that three samples heated for the same time may show enormous variations.

Milk Sugar Pol.	1.	2.	3.	4.	5.	6.	7.
Original	5.09	5.20	5.10	5.26	5.09	4.75	5.30
Heated about 2 hours	4.54	4.56	4.39	4.66	$\left. \begin{matrix} 3.88 \\ 4.40 \\ 4.37 \end{matrix} \right\}$	$\left. \begin{matrix} 3.72 \\ 3.64 \\ 3.98 \end{matrix} \right\}$	4.84
			4 hr.	4.24		$\frac{1}{4}$ -hr.	5.20

We have quoted the bulk of the results for two hours' heating and not for less times, simply to show the change on heating in a distinct manner. The numerous methods of milk sterilisation by heating will all produce the same change, though possibly to a different degree. A figure is given for $\frac{1}{4}$ -hour in No. 7, as the results there are means of about 50 polariscope readings, and are probably accurate. This shows that even in $\frac{1}{4}$ -hour the change may be detected by a delicate polariscope.

To show that the reducing power on Fehling's solution is constant, we may quote the following experiments:—

Original	5.31 (grav.)	4.41 (pol.)	4.92 (grav.)
Heated 2 hours ...	5.22 (grav.)	$\overbrace{4.56 \text{ grav. } 4.45 \text{ grav.}}$	4.88 (grav.)

It is well known that milk sugar has, besides its normal and bi-rotation, a half rotation (c.f. Vieth, *ANALYST*, xi., 141); we thought it possible that, on heating, a quantity of the half-rotating modification might be formed: Schulze and Tollens (*Annalen*, 271, 49) have shown that sugars dissolved in dilute ammonia always exhibit the normal rotation. We found, however, that no change was produced by adding ammonia, nor by allowing our solutions to stand some hours. It is noticed that when milk is heated for a long time to 100°, a brown colour is produced, and we have found that the rotation varies to some extent inversely as the colour. This is probably due to the formation of a "caramel," and it is well known that "caramel" prepared by heating dry sugar to 170° is inactive. Incidentally we may mention that in a sample of milk heated to 85° for a few minutes the albumen found by Sebelein's method was 0.19%.

Milk sugar in solution as in milk differs from dry milk sugar as it is caramelized at 100°. Weisberg (*Chem. Centr.*, 1892, ii., 458) has noticed a decrease in the rotary power of cane sugar in solution by several hours' heating.

THE BOILING POINTS OF SALT SOLUTIONS.

BY H. DROOP RICHMOND.

WISHING to find a convenient method of keeping a water bath at a constant temperature a little above 100° C., it struck me that by adding a proportion of salt (sodium chloride) to the water inside and keeping the level constant by the automatic addition of water, the desired effect might be obtained.

I found, however, that the temperature attained did not agree with that according to Legrand's tables (*Ann. Chim. Phys.*, 1835 [2], lix., 423) and therefore redetermined them. My apparatus consisted of a flask with a layer of pumice stone at the bottom, containing a considerable quantity (100 c.c.) of salt solution; through the cork passed a 5 c.c. pipette and a tube with a side tube attached, through which the thermometer was passed; the end of the pipette and the bulb of the thermometer were in close proximity.

The mode of operation was as follows:—The solution was boiled, and the thermometer was read at any point; immediately the pipette was washed out with the solution two or three times, and the thermometer again read to one-tenth degree; 5 c.c. were then withdrawn and placed in a stoppered weighing bottle, and the thermometer again read; the mean of the readings was taken as the temperature at the time of withdrawal of the solution. After cooling, a portion of the solution was weighed out into a 500 c.c. flask, and 100 c.c. were titrated with silver solution, with potassium chromate as indicator, care being taken that about 45 c.c. of silver solution were required in each case; the silver solution was standardised on such quantities of pure sodium chloride as took 45 c.c. of solution. The percentage of sodium chloride was calculated from the data obtained in each titration. The thermometer readings were corrected to 760 m.m. by taking the boiling point of water at the same time; as the stem was at 100° C. the correction for the mercury therein was very small.

The molecular percentages of sodium chloride were also calculated.

No.	Boiling point.	NaCl. %.	NaCl. Mol. %.	B. P. (calc.)
1	... 102.2°	7.6	2.47	102.15°
2	... 103.0°	11.0	3.63	103.16°
3	... 104.2°	14.9	5.11	104.44°
4	... 104.8°	16.1	5.58	104.85°
5	... 106.1°	18.8	6.66	105.79°
6	... 107.1°	22.3	8.12	107.06°
7	... 107.7°	24.0	8.87	107.71°
8	... 108.7°	26.0	9.76	108.49°
9	... 109.5°	28.7	11.04	109.61° (saturated solution)

My results are about 1° C. higher than Legrand's; the boiling point appears to be a direct function of the molecular percentage, and the boiling points calculated by adding the molecular percentage divided by 1.15 to 100° C. are within the limits of reading of the thermometer used. It is interesting to note that a saturated solution has nearly the composition $\text{NaCl} \cdot 8\text{OH}_2$. If time permits, I hope to investigate the boiling points of other salts to see if it is a general law that the boiling points are direct functions of the molecular composition of the solution, and also to determine the densities at the boiling point. If, however, any other observer having greater facilities than I have will do this, I shall be only too pleased to leave the work to him; a large and reliable series of determinations would be extremely valuable.

Wullner (*Ann. Chim. Phys.*, **190**, 564) finds the boiling point to vary as the amount of salt dissolved. Nicol (*Phil. Mag.* **22**, 502) has attacked the question from a different point of view, and his conclusions do not apply to boiling solutions.

Papers were then read, entitled "Points in the Analysis of Condensed Milk," and "Note on the Detection of the Adulteration of Fresh Milk by Diluted Condensed Milk," both by Messrs. Richmond and Boseley.

(*Conclusion of the Society's Proceedings.*)

The Determination of Nitrogen in Nitrates by Reduction. T. F. Schmitt. (*Chem. Zeit.*, 1893, xvii, 173.)—The method is a modification of that already described by the author (*THE ANALYST*, 1893, 92), in which a mixture of powdered zinc and iron is used as a reducing agent. The process is carried out as follows:—10 grms. of the nitrate are dissolved and made up to 500 c.c. 10 c.c. of glacial acetic acid and 10 grms. of the metallic powder are placed in a flask of a capacity of about 750 c.c., and 25 c.c. of the solution of the nitrate added. The flask is covered during the reduction to prevent loss by spraying, and after solution is complete (which is the case in about ten minutes) the contents of the flask is diluted with 200 to 300 c.c. of water, 30 c.c. of caustic soda of sp. gr. 1.25 added, and the whole distilled as in the Kjeldahl process. It must be noted that it is essential that the iron be finely divided; it is mixed with the powdered zinc in equal parts. The total nitrogen can be determined in guanos and nitrate mixtures by the following simple alteration in procedure:—1 gm. of the substance is dissolved in water, 5 c.c. of glacial acetic acid and 2 to 3 grms. of the metallic powder added, and the whole gently heated for ten or fifteen minutes. After the contents of the flask have cooled, 25 c.c. of sulphuric acid are cautiously added in small portions, undue frothing being restrained by the addition of a fragment of paraffin wax. The acetic acid is then driven off by heating, and the remaining contents of the flask heated until the organic matter is completely decomposed, as in the Kjeldahl process, which happens in about two hours. Neutralisation and distillation are then proceeded with in the ordinary manner. The method is applicable to the determination of nitrates in drinking water, provided nitrites and ammonia be absent.

B. B.

The Presence of Ammonia in Zinc Dust. F. Robineau and G. Rollin (*Monit. Scient.* 1893, vii., 138, *through Chem. Zeit.*)—Commercial zinc dust appears always to contain ammonia, a fact the importance of which is obvious when it is used for reducing nitrates (see preceding abstract). On extracting zinc dust with warm water only a trace of ammonia is removed, as can be recognized by the use of Nessler solution, but on boiling the extracted material with caustic potash a further portion of ammonia is given off. Granulated zinc that has become partially oxidised by exposure to moist air, also contains ammonia. The complete removal of ammonia is conveniently effected by washing the zinc dust with boiling 1 per cent. sulphuric acid, after which treatment no ammonia is given off on heating with potash. As exposure to the air for two days

suffices to cause the reappearance of ammonia in purified zinc dust, it is necessary to prepare it as it is wanted, and use it immediately.

B. B.

Kreis' Modification of the Reichert-Meissl Process. (*Chem. Zeit.*, 1892, xvi. 1394; 1893, xvii. 395, 468.)—At the Annual Meeting of the Swiss Association of Analytical Chemists, held in September, 1892, Kreis announced that butter-fat and the fats used for margarine can be completely hydrolysed by strong sulphuric acid at 30-32°C., and that, after dilution, the volatile acids may be directly distilled over and estimated as usual. He noted that while the results obtained with pure butter by this method agree well with those given by the old saponification process, those obtained with margarine are much higher. This appeared to him to open the door to a new method, more delicate than we at present possess, for detecting margarine in butter.

Pinette (*Chem. Zeit.*, 1893, xvii. 395) has pointed out that the formation of sulphurous acid during the hydrolysis renders Kreis' modification quite impracticable as he stated it. By oxidising the sulphurous acid before distillation better results are obtained. Pinette proceeds as follows:—Five grams of the filtered butter-fat are melted in a convenient flask, and 10 c.c. of strong sulphuric acid are added with continual agitation. The fat immediately dissolves with evolution of sulphurous acid; 150 c.c. of water are added, and a strong solution of potassium permanganate until the pink colour is permanent for several seconds; the liquid may then be distilled. The rapidity of the method is greater than that of the Reichert-Meissl process, and Pinette is also of opinion that a comparison of the two processes will form a useful indication of margarine.

Prager and Stern (*Chem. Zeit.*, 1893, xvii. 468) propose to eliminate the sulphurous acid by a stream of air, succeeded by one of carbonic acid, and proceed as follows:—5 grams of the butter-fat are brought into a litre flask, 10 c.c. of strong sulphuric acid are added, and the flask is kept for ten minutes at 30-32°C. with constant agitation. When the liquid is cold, air is bubbled through it until the odour of sulphurous acid has disappeared, 100 c.c. of water are added, with precautions against rise of temperature, and carbon dioxide is bubbled through for ten minutes. This is then displaced by a stream of air for another ten minutes, the delivery tube is washed into the flask with 50 c.c. of water and the distillation is effected. The following results are quoted:—

			C.C. of $\frac{N}{10}$ alkali required by five grams of butter-fat.	
			Reichert-Meissl.	Prager-Stern.
Sample <i>a</i>	...	..	29.86	29.60
„ <i>b</i>	...	...	30.23	29.65
„ <i>c</i>	...	..	28.34	27.76
„ <i>d</i>	...	...	28.20	28.10

The authors do not comment on the possibility of loss of acids other than sulphurous in the stream of air, but they admit that further investigation is requisite to render the Kreis suggestion serviceable.

A. G. B.

Determination of Fatty Oils in Mineral Oils. J. Klimont. (*Chem. Zeit.* 1893, xvii. 543.)—Fifteen grms. of the oil which is to be examined for the presence of small quantities of fatty oil are saponified in a flask, holding about 400 c.c., with 100 c.c. of an alcoholic solution of caustic potash (not soda), by boiling under a vertical condenser for one or two hours. The contents of the flask are then diluted with an equal bulk of water and filtered through a wet filter. The residue of mineral oil is well washed, the filtrate exactly neutralised with hydrochloric acid, allowed to stand in a separating funnel until thoroughly cold to get rid of any remaining mineral oil, and then extracted with petroleum ether to complete the removal of the unsaponified portion. The aqueous liquid containing all the fatty oil as soap, is concentrated to 100 c.c., precipitated with a solution of calcium chloride, and the resulting lime soap collected on a weighed filter, washed with the least possible quantity of cold water, dried and weighed. It is then ignited, and the residual lime weighed again, giving the fatty acids. These can be calculated to glycerides by adding the weight of the glyceryl radicle displaced by the lime, the necessary value being obtained by multiplying the weight of the CaO found by 0.774. The process is adapted for the determination of small percentages of fatty oils, *e.g.*, under 5 per cent., but is not suitable for larger amounts, as the lime soap encloses a good deal of potassium chloride and calcium chloride, and cannot be satisfactorily washed.

B. B.

The Separation of Ferric Oxide and Alumina. H. Borntrager. (*Zeits. anal. Chem.*, 1893, xxxii., 187, through *Chem. Zeit.*)—The mixed oxides of iron and aluminium are ignited, weighed and dissolved in hydrochloric acid, neutralised with caustic potash (not soda), and precipitated hot with potassium oleate, avoiding an excess of the precipitant as it dissolves the iron and aluminium soaps that are thrown down. The precipitate is filtered, washed with hot water, and dried over calcium chloride to remove the bulk of the water which it contains. Hot petroleum is poured over the dried precipitate, and the ferric oleate is thus dissolved, leaving the aluminium oleate, which can be ignited, and the resulting alumina weighed. The solution of ferric oleate in petroleum is ignited in a platinum dish, and the residue of ferric oxide also weighed. The process can be used for the separation of ferrous iron from aluminium, as ferrous oleate is soluble in petroleum like the ferric salt.

[NOTE BY ABTRACTOR.—The usefulness of the method is impaired by the circumstance that alumina, if ignited sufficiently strongly to become anhydrous, is insoluble in hydrochloric acid, so that the mixed oxides would have to be dissolved by treatment with potassium bisulphate, or some equivalent method.]

B. B.

Detection of Monovalent Alcohols. B. V. Bitto. (*Chem. Zeit.* 1893, xvii. 611.)—The usual methods, such as that depending upon the iodoform reaction, for detecting alcohol, are not satisfactory, since they do not apply to alcohol only, but also to

other analogous substances. The reaction about to be described, although not particularly delicate, is claimed by the author to be definitely characteristic of monovalent alcohols. 0.5 grm. of methyl violet is dissolved in a litre of water, and 1 to 2 c.c. of this solution together with 0.5 to 1 c.c. of a solution of an alkaline polysulphide, is added to the liquid to be tested. If a monovalent alcohol be present, the liquid remains clear, but becomes cherry or violet red. It must be noted that a fair amount of the liquid to be tested should be taken, and that the coloration alters on standing. In the absence of a monovalent alcohol the liquid becomes greenish-blue, and after a time deposits reddish-violet flocks, the liquid itself, at the same time, becoming yellow. The reaction appears to take place with substances containing the group $C(OH)$. The behaviour of the following bodies has been tried:—

Substance.							Coloration.
Methyl alcohol	...	...	...	...	...	...	Cherry red.
Ethyl alcohol	...	...	...	...	...	...	"
Normal propyl alcohol	...	...	...	...	...	...	"
Iso-propyl alcohol	...	...	...	...	...	...	"
Tertiary butyl alcohol	...	...	...	...	...	...	Violet red.
Iso-butyl alcohol	...	...	...	...	...	...	"
Iso-butyl carbinol	...	...	...	...	...	...	"
Allyl alcohol	...	...	...	...	...	...	"

Divalent and trivalent alcohols, carbohydrates, acids, aromatic compounds, phenols, etc., do not give the reaction. B. B.

The Analysis of Rubber Goods. D. Holde. (*Mitt. könig. tech. Versuchs.*, 1892, x., 315, through *Chem. Zeit.*)—The author supplements the work of Henriques (*THE ANALYST*, xviii., 13), by the suggestion of a method for the determination of the oil in rubber, and its surrogates other than that which has been "vulcanised" by treatment with sulphur chloride. The unaltered oil is soluble in ether-alcohol, whereas rubber and its surrogates are not much attacked by this solvent. It is true that pure caoutchouc yields three to four per cent. of an oily substance when extracted with ether-alcohol, and a correction is therefore necessary. The mixture adopted consists of four parts of ether and three of alcohol, and is best used by allowing the rubber to stand in the ether for a day, and become swollen to a pulp, the mass being heated, if necessary, and then adding the alcohol. After the removal of the fatty oil, the analysis can be proceeded with in the manner indicated by Henriques (*loc. cit.*) B. B.

Coffee Glazing. F. Filsinger. (*Chem. Zeit.*, 1893, xvii., 498.)—In order to give a lustrous aspect to roasted coffee, a liquid is now in use which is free from colour, taste, and smell, and is clear and oily in appearance. It has a specific gravity of 0.868 at 15° C, and burns with a sooty flame, leaving no fixed residue. It absorbs no iodine when

treated by Hübl's method, and is but little affected by chromic acid and concentrated sulphuric acid, taking only a slight brown colour when treated with the latter reagent. It mixes in all proportions with petroleum ether. It therefore appears to be nothing but a highly purified petroleum oil, and must be considered as an illegitimate addition.

B. B.

The Volumetric Estimation of Acetone. F. Robineau and G. Rollin. (*Monit. Scient.*, vii., 272, through *Chem. Zeit.*)—The usual method for the determination of acetone rests upon the conversion of that body into iodoform, which is weighed. The method is slow and inexact, owing to the loss of iodoform by volatilisation. These inconveniences are remedied by the author, who converts the method into one in which the estimation is volumetric. The acetone, having been obtained in a solution free from substances that also give the iodoform reaction, is treated with excess of potassium iodide and caustic soda, and a standard solution of sodium hypochlorite run in until the conversion of the acetone into iodoform is complete, as evidenced by the appearance of a blue colouration when a drop of the liquid, being titrated, is placed upon a piece of starch paper, moistened with a solution of starch and sodium bicarbonate. A mere trace of sodium hypoiodite, in the presence of caustic soda, gives a blue colour with starch paper that is saturated with sodium bicarbonate. Certain precautions are necessary to obtain exact results. The liquid containing the acetone must be thoroughly alkaline with caustic soda, as otherwise more hypochlorite than the normal amount will be necessary to convert the acetone into iodoform. An excess of potassium iodide must be present. The dilution of the solution titrated must be nearly the same in all cases, and a similar strength of hypochlorite must always be used. The operation should not be conducted in a strong light, and the solution must be constantly stirred during titration. The standardisation of the hypochlorite is effected by means of pure acetone.

B. B.

Rapid Process for determining Organic Nitrogen in Urine. A. Petit and L. Monfet. (*J. Pharm. Chem.*, 1893, xxvii. 297, through *Chem. Zeit.*)—The process rests on the same principle of that of Kjeldahl, but with the modification that the ammoniacal nitrogen is finally estimated by means of an alkaline hypobromite. 10 c.c. of urine are digested with 5 c.c. of sulphuric acid in the usual way, with the addition of mercury, after which the solution is diluted with 20 c.c. of water and caustic soda added in small quantities at a time, the flask being kept cool with water. The liquid is left slightly acid, poured into a 50 c.c. flask, made up to the mark, and 10 c.c., corresponding to 2 c.c. of the urine, introduced into a urinometer and mixed with a solution of sodium hypobromite, made by adding 10 c.c. of bromine to 90 c.c. of strong soda lye and diluting with 75 c.c. of water. In the case of urine containing albumen the digestion with sulphuric acid is somewhat longer, taking about half-an-hour. For alkaloids containing a pyridine or quinoline nucleus, a digestion of $1\frac{1}{4}$ to 2 hours is requisite. For pyridine itself or analgelsin even 4 hours is not sufficient to ensure complete oxidation.

B. B.

The Oils from the Kernels of Apricots, Cherries, Plums and Peaches, and their possible use as Adulterants of Almond Oil. C. Micko. (*Zeits. österr. Apoth. Verein.*, 1893, xxxi. 175.)—The above-named oils are all fluid at the ordinary temperature and have a yellow colour and a mild pleasant taste recalling that of almond oil, so that they could not be detected in admixture with it by simple inspection. The author has therefore determined the following constants by means of which discrimination is practicable:—

		Apricot.	Cherry.	Plum.	Peach.
For the oils	Specific gravity ...	0.92108	0.92850	0.91949	0.92147
	Acid number ...	0.64	1.11	0.55	0.52
	Saponification ...	193.11	193.36	191.55	191.11
	Iodine number...	108.07	114.30	100.20	99.71
For the insoluble fatty acids ...	Saponification number...	194.04	189.04	200.47	200.87
	Iodine number ...	102.64	104.33	104.21	101.93
	Melting point...	13.4—18°C	16—20.6	*12.4—18.1	10—18.9
	Mean molecular weight	288.60	296.23	279.34	278.78

The iodine absorption is therefore the best single criterion for these oils. Certain colour reactions are also given as an aid in detecting them. With nitric acid of specific gravity 1.4, apricot and plum oil give an orange colour, peach oil at first a yellowish brown then a dirty orange; cherry oil gives a dark brownish red colour and the aqueous layer becomes bright red on standing. Bieber's reagent (a mixture of concentrated sulphuric acid, crude nitric acid and water in equal parts), gives a beautiful peach-red colour with apricot oil, a brown, darkening on standing, with cherry oil, a rose colour with plum oil, and with peach oil no colouration for the first half-hour, but a bright brown colour after the lapse of some hours. It has hitherto been generally accepted that peach oil gives a peach-red colour, but according to the author's experiments this is not the case, that colour being characteristic for apricot oil.

B. B.

Phosphates in Milk. Duclaux. (*Ann. Inst. Past.*, 1893, vii. 2, through *Chem. Zeit.*)—According to the experiments of the author, the phosphates in milk are present in suspension and solution. The former are phosphates of iron, aluminium, magnesium and calcium, while the latter consist of nearly equal molecular quantities of phosphate of sodium and phosphate of calcium, held up by sodium citrate. The result of the examination of various kinds of milk is that there is about twice as much calcium phosphate in suspension as in solution, and that the composition of the ash of milk of the most diverse origins is very nearly identical. An attempt to produce a "phosphate milk" by adding phosphates to the fodder has proved quite ineffective, the milk containing no more phosphate than ordinary qualities.

B. B.

* 1.24 in original

The Analysis of Aniline Oil. H. Reinhardt. (*Chem. Zeit.*, 1893, xvii. 413, 414.)

—The method used by the author depends upon the following facts:—

(1.) Aniline, when treated with a mixture of potassium bromide and bromate in dilute acid solution, yields the tribromo derivative, while ortho- and para-toluidine give dibromo substitution products.

(2.) On precipitating a hydrochloric solution of the three bases with oxalic acid, under suitable conditions, para-toluidine is first thrown down, then aniline, while ortho-toluidine remains in solution. The author's control analyses have been carried out with the pure bases prepared by himself, as the commercial products are not sufficiently good for the purpose.

I. The determination of aniline and of the total amount of the two toluidines in mixtures of aniline and either or both toluidines.

The brominating solution is prepared from a solution of 480 grms. of bromine, 336 grms. of caustic potash of 100 per cent. strength, dissolved in 1 litre of water, the whole being boiled for two or three hours in order to get rid of hypobromite, and finally diluted to 9 litres. For the standardisation of the solution as well as for the actual determination of the oil, about 1.5 to 2 grms. of the oil are taken, dissolved in 100 c.c. of hydrobromic acid of specific gravity 1.45 to 1.48, diluted with a litre of distilled water and titrated with the bromine solution until there is present sufficient excess of bromine to give a colouration with paper that has been soaked in a mixture of potassium iodide and starch. The bromine solution retains its strength fairly well, a very trifling difference being perceptible after it had been kept a fortnight.

That the method is reasonably exact is shown by the fact that the results for pure oils ranged from 99.90 to 100.06 per cent. The content of aniline is reckoned by the following equation:—

$$x = 2.3777vt - 1.3777a$$

where a is the quantity of oil taken, x the amount of aniline it contains (that is to be determined), v the volume of bromine solution used in c.c., and t the amount of aniline to which one c.c. of the bromine solution corresponds as determined in the process of standardisation. The weight of aniline found in this manner, deducted from the total quantity of oil taken for analysis, obviously gives the amount of the two toluidines, and the percentages can then be calculated in the ordinary way. The result of test analyses are very satisfactory.

II. The determination of para-toluidine in the presence of aniline or ortho-toluidine or of both bases.

In order to obtain accurate results by the oxalate method mentioned above, more oxalic acid must be used than suffices for the precipitation of the para-toluidine present. A preliminary trial must therefore be made to gather some idea of the content of para-toluidine. The quantity of oil taken for analysis varies from 10 to 100 grams, according to its content of the body to be estimated. In general, 100 grams of the oil are mixed with 106 grams of hydrochloric acid, free from sulphuric acid, and containing about 31 per cent. of HCl, and the mixture added to a solution of the amount of oxalic acid (free from lime) that has been found necessary by the preliminary trial, dissolved in ten times its weight of water. The resulting liquid should remain clear even when a large percentage of para-toluidine is present; crystallisation of the oxalate takes place after stirring and is completed by allowing the solution to stand for 48 hours. The oxalate is then freed from the mother liquor by filtration with the pump, and washed three times with distilled water, 25 c.c. being used for each washing. The oxalate is then decomposed by being added to a solution of caustic potash (190 c.c. of potash, 45°B, and 200 c.c. of water), and after cooling, the precipitated oil collected and weighed. It contains a certain amount of aniline, which is determined by titrating it with bromine solution in the manner already described. A simple calculation gives the percentage of para-toluidine, to which a constant correction of + 2 per cent. must be made for the prescribed conditions of separation. The test analyses given are very satisfactory. The use of these methods of analysis shows that the commercial products purporting to be pure oils, often contain considerable quantities of their isomers or homologues.

B. B.

On Electrolytic Methods of Analysis. F. Oettel. (*Chem. Zeit.*, 1893, xvii, 173-174.)—The author performs the very necessary task of calling attention to the want of precision in the methods commonly adopted for describing the conditions under which electrolytic processes should be carried out. It is the custom simply to state that with a current of so many amperes the metal under consideration separates out in a state of purity, and in a compact and weighable form. This is quite insufficient to enable another analyst to repeat the operation with the certainty of success, as the most important factor in the separation of a metal in a given condition, is not the strength of current, but the current density, that is to say the number of amperes per unit of area of the electrode. He therefore proposes that all who record the results of investigations of this kind should select the square decimeter as the unit of area, and state that the separation was effected by a current of so many amperes per square decimeter of cathode. It is important to note that the current density will not be uniform over the surface of the cathode if the latter be in the shape of a cylinder, for example, surrounding a wire which serves as the anode. In this case the density will be greater on the inner surface, and more metal will therefore be deposited there. It suffices in such cases to state the average current strength referred to the whole immersed surface of the cathode, and to mention the shape and disposition of

the cathode, the object being not the attainment of rigid accuracy in stating the current density, but the indication of it in such manner as will enable another chemist to reproduce the conditions which experience has shown to be successful. Another objectionable practice in recording the results of electrolytical researches, is that of stating the current in c.cs. of explosive gas given off in a voltameter in circuit. The strength should always be given in amperes, about which there is no ambiguity. It may not be needless to add, in view of the mistakes that are often made, that the measuring instrument, whatever it be, must be inserted in circuit while the analytical operation is in progress, as a wholly false result will be got by substituting it for the depositing cell when the latter is removed. A convenient form of amperemeter is a cell containing two copper plates immersed in a solution of 15 parts of copper sulphate, 5 of sulphuric acid, and 100 of water. The gain of weight of the cathode is the measure of the current that has been employed, and is reduced to amperes by the use of the equation,

$$a = \frac{w}{t \times 0.0197}$$

where a represents the current in amperes, t the time in minutes, and w the weight of copper in grams. The advantage of the copper voltameter is that as there is no reduction of metal, but only its transference from anode to cathode, the instrument consumes only the energy required to overcome its internal resistance, which may be small, and thus the drop of voltage which it occasions is in no way considerable.

The author also calls attention to the desirability of stating the voltage of the current used, although this is of less importance than the current density. B. B.

[NOTE BY ABTRACTOR. It has been recently shown that the electrolytic separation of metals, the heat of combination of whose salts differs appreciably, can be conveniently effected by the use of a current of regulated pressure, which may be arranged to throw down those whose heat of combination is lowest, while leaving the others in solution. The voltage of the current employed, therefore, cannot be neglected.]

GLAMORGANSHIRE QUARTER SESSIONS.

THE SALE OF BAKING POWDER CONTAINING ALUM.

(Compiled from Reports in *The Cambrian* and elsewhere.)

WEDNESDAY, APRIL THE 12TH, 1893.

ON the bench:—J. Coke Fowler, Esq., Chairman; his Honour Judge Gwilym Williams, Vice-Chairman; and Edward Daniel, Esq.

This was an appeal case brought by James James, grocer, Blaenrhondda, who was convicted at Pontypridd on February 13th last, under the Sale of Food and Drugs Act, for selling baking powder, known as the "Excelsior" baking powder, containing injurious ingredients. Mr. D. Brynmor Jones, Q.C., M.P., Mr. T. C. Blofield (specially retained) and Mr. Arthur Lewis (instructed by Messrs. Tillett and Co., Norwich), appeared for the appellant, and the respondent

(Supt. Evans, Pontypridd) was represented by Mr. David Lewis and Mr. Rhys Williams (instructed by Mr. W. E. R. Allen, Cardiff).

In opening the case for the respondent, Mr. David Lewis said he thought there would be no dispute as to the ingredients of the baking powder in question. There was no definition as to what baking powder really was, but when not adulterated with alum or anything else, it was entirely bi-carbonate of soda. The question of the constitution of Borwick's baking powder had often been brought up. That was made in a certain way, not of alum but of tartaric acid. When he told them that tartaric acid cost over £6 per hundredweight, whilst alum was only five shillings, they would readily see that the temptation to use alum was very great indeed. Therefore, it was desirable that the public should be protected in this matter. The learned counsel then explained the chemical changes which took place when alum came in contact with the saliva and gastric juice of the mouth and body of a person, with a view of showing that the chloride of aluminium which was formed was injurious to the system, by reason of its retarding the digestive operations.

Superintendent Evans was then called and examined by Mr. Rhys Williams. He said he was stationed at Pontypridd, and was an inspector appointed under the Food and Drugs Act. He then went on to depose to the purchase of the baking powder from the appellant on December 10th last. According to Dr. Morgan's (public analyst) certificate the substance contained alum to the extent of 39 per cent., which was, in Dr. Morgan's opinion, prejudicial to health. Witness went on to speak of subsequently making bread with the powder in it, and submitting the loaves to various medical gentlemen for analyses.

Dr. W. Morgan (Public Analyst) was next called, and stated that taking a teaspoonful of the baking powder at 120 grains, a four-pound loaf would contain 360 grains, four-tenths of which would be alum. When water was added to the baking powder, chemical action took place between the potash alum and bi-carbonate of soda, and produced hydrate of alumina, sulphate of soda, sulphate of potash, carbonic acid gas and water. The quantity of hydrate of alumina might be taken as one-sixth of the alum, or six grains per pound loaf. When the bread was eaten the hydrate of alumina was dissolved by the gastric juice in the stomach, in consequence of the hydrochloric acid and pepsin contained in the gastric juice. The result was that chloride of aluminium was formed from the contact between the hydrate of alumina and free hydrochloric acid, and that chloride of aluminium was noxious to the stomach. Detailing a personal experiment made a few days ago, Dr. Morgan said that he and his son partook of an ordinary mid-day meal. His son drank some hydrate of alumina in water with his food, but he, himself, drank nothing. The hydrate of alumina was obtained from the baking powder in question. A short time afterwards artificial vomiting was produced, and hydrate of alumina was then added to the contents of his own stomach. Both vomits were then dialysed and chloride of aluminium was found in each, thus proving that the hydrate of alumina was in a soluble state. That experiment had since been twice repeated, and on the first occasion was made under the supervision of Drs. Griffiths and Brooks. Cross-examined by Mr. Brynmor Jones, Dr. Morgan admitted that though an oxide might be injurious, a compound might be harmless, as was shown in the case of common salt, which was purely chloride of sodium, yet chlorine was poisonous. In the making of bread, however, he was of opinion that alum would produce the same effect as alumina.

Mr. Otto Hehner, called, said he was past President of the Society of Public Analysts and Public Analyst to several counties in England, and had made food analysis his special study for the past twenty years. In his opinion such baking powder as that in question was injurious to health by reason of the aluminium salts it contained. He was of opinion that hydrate of alumina was soluble in the stomach. Cross-examined by Mr. Brynmor Jones: Witness had written a good deal upon food analysis, and he, as well as many medical men, considered alum an abomination in bread. Alum and alumina were two distinct substances producing the same result.

Professor Claude Thompson, Professor of Chemistry in University College, Cardiff, gave substantially the same evidence, and detailed experiments he had made, coming to the conclusion that hydrate of alumina was soluble in the stomach. Cross-examined by Mr. Brynmor Jones: Witness said that when alum was dissolved in water there was no chemical change. He could not say what would be the result if the alum when mixed with water were heated.

Professor Wndham Dunstan, M.A., Oxford, F.I.C., Professor of Chemistry at St. Thomas's Hospital, London, &c., was next called, and he stated that the hydrate of alumina, dried at 212° F., was insoluble in water, but was soluble in the gastric juice of a dog diluted to 0.2 per cent. strength, and that the gastric juice of a dog diluted to this extent dissolved hydrate of alumina from bread baked with the powder. That the hydrate of alumina in the bread

interfered with the digestion of starch by ptyalin (diastase was used in the actual experiment) and with both peptic and pancreatic digestion. Further, that dilute solution of carbonate of soda of 0.3 per cent. strength, the strength of the alkali in intestinal juice, would dissolve hydrate and phosphate of alumina, either freshly precipitated or dried at 212° F., so that absorption of aluminium compounds could occur from the intestine. This was confirmed by his observation that aluminium compounds occurred in the urine of a man who had daily taken 15 to 30 grains of the hydrate dried at 212° F. On one occasion the taking of this quantity of hydrate produced vomiting. In the course of a subsequent re-examination, Professor Dunstan said he had heard Dr. Morgan relate his experiments, which in his (witness's) opinion made the matter more conclusive, and were very valuable experiments.

Dr. Lauder Brunton was next examined, and stated that it was a fact that the action of alum caused disturbance of the digestive functions. He was of opinion that Dr. Morgan's experiments afforded conclusive evidence that the hydrate of alumina in the stomach changed into chloride of aluminium, the cause of the change being the hydrochloric acid. The presence of alumina in the stomach he considered injurious to the human system. In cross-examination by Mr. Brynmor Jones, the witness stated that alum taken in small quantities had an astringent action, but when a larger quantity was taken, it had an irritant action. It was the constant use of baking powder containing alum that proved so injurious to a person. Hydrate of alumina had the same effect as alum, because the base was the same in one respect.

The Court then adjourned till Thursday.

THURSDAY.

Dr. Rhymer Marshall, Doctor of Science of the University of Edinburgh, and Fellow of the Chemical Society, but now of University College, Cardiff, was the first witness called and examined by Mr. David Lewis. He agreed with the previous witnesses for the respondent that hydrate of alumina was passed into the stomach by the eating of alum, and that that hydrate was soluble in the gastric juices and formed chloride of aluminium. In Scotland bi-carbonate of soda was frequently used as a baking powder.

Dr. Thomas Druslyn Griffiths, of Swansea, stated that there were two effects—local and constitutional—produced when hydrate of alumina was converted into chloride of aluminium in the body, whilst the result was highly irritating to the internal organs. Cross-examined by Mr. Brynmor Jones: He had never treated a patient suffering from the effects of this irritant, and had declined to give evidence in the present case on either side until he saw Dr. Morgan's experiments.

Dr. Ebenezer Davies, Medical Officer of Health for Swansea, gave similar evidence as that previously given, submitting that six grains of hydrate of alumina retarded the progress of digestion.

Dr. Thomas Henry Morris, Bachelor of Medicine of the Glasgow University, now practising in the Rhondda Valley, stated that prior to this case he was not aware that alum was used in baking powder. Taking the proportion as used in the "Excelsior" baking powder, it would be highly injurious to health.

Dr. W. Williams, Master of Arts, and Bachelor of Medicine, of Oxford, &c., and Medical Officer of Health to the Glamorgan County Council, was the last witness examined on behalf of the respondent's case. He said that an antidote in a case of poisoning by alum must be an alkaline one, because the acid was the principal substance to be neutralised.

The Court then adjourned for luncheon.

Directly after luncheon, Mr. Brynmor Jones opened the case for the appellant, his address lasting nearly two hours. At the outset he read section 3 of the Act under which the proceedings were instituted, and said the only evidence given by his learned friend of an offence under the Act, was that James James, the appellant, sold a packet of baking powder. The case was, therefore, not the same as if the appellant had sold a loaf of bread which had been manufactured with the aid of the powder. Now was baking powder food? He would submit that it was not used for the food of man.

The Chairman: You must take it in connection with the interpretation clause of the Act.

Mr. Jones replied that he did so, and went on to say that what was meant by food in the Act was substances taken for the purpose of sustaining the life of human beings. The mere fact that some substance used for the making of food ready for reception into the stomach of a person did not render such articles, mixed in the preliminary stage of the manufacture of food, food itself.

The Chairman said he felt the force of the argument and could not see how the Bench could possibly hold that baking powder in itself was food. But the interpretation clause certainly seemed to suggest a latitude. The Act of Parliament was a remedial one passed *pro bono publico*, and that being the case there ought to be a liberal interpretation of the section.

Mr. Jones opposed to that that the Act of Parliament was not to be construed extensively, and went on to show that it was a penal statute. But if the learned Chairman's construction was accepted, that baking powder was an article of food, what, the learned counsel asked had been "mixed" with it, in order to constitute an offence under the section? The evidence for the prosecution did not show that anything had been mixed with the powder.

The Vice-Chairman: Baking powder need not necessarily contain alum.

Mr. Jones: It is absolutely necessary, according to the evidence already called, in order to make baking powder. Bi-carbonate of soda without an acid was not a baking powder at all.

The Vice-Chairman: But it is not a baking powder until allied with bread.

Mr. Jones: Then it is only an offence if we had sold a loaf.

The Vice-Chairman believed that baking powder, in the abstract, was an article of food, because it became a part of the loaf. The case for the prosecution was that some portion of the baking powder remained as a residuum, and that it became a part of the bread which went into the stomach. If so, surely the powder was food. If the learned counsel could persuade him that in the process of kneading the dough or baking it, the whole of the alum, which was admitted to be in the powder, was disposed of, then there would be no case.

Mr. Jones: The evidence shows that the whole of the alum in the baking powder is necessarily got rid of, and a new substance created.

The Vice-Chairman: No, it is alum in another form, and would not be there unless you put the alum in the flour.

Mr. Jones differed, and went on to refer to the composition of water with a view of showing that the hydrate of alumina was no more a form of alum than water was a form of either oxygen or hydrogen, after which he read a decision of the Recorder of Cambridge (Mr. Bullwer, Q.C.), that baking powder was not an article of food. He further submitted that there was no evidence adduced to show that the baking powder was injurious to health, but only that it might be. According to the form of the indictments and summonses which were usual in their criminal practice, the offence must always be charged with certainty.

Mr. Blofield concurred with all that Mr. Brynmor Jones had said, but the objections were overruled, the Bench considering there was a case to answer.

Mr. Brynmor Jones then continued his address, and said the use of baking powders had gone on for a period of fifty years, as it was well known that unleavened bread was very indigestible. He explained that the object of baking powders was to liberate carbonic acid gas and so cause the dough to rise. One of the earlier substances used as a baking powder for that purpose was made from carbonate of soda and tartaric acid, whilst a later method was the introduction into dough of carbonic acid gas by mechanical appliances, as was the case with the aerated bread. But alum baking powders had been known for many years, and alum was quite as essential a substance to use as tartaric acid.

The Chairman: But you would not say that alum tends to increase fermentation?

Mr. Jones: Yes; only indirectly it produces carbonic acid gas. Learned counsel then went on to say that Dr. Dyke, one of the witnesses whom he should call, had used an almost identical composition as the present baking powder in his family for a great many years. That powder Dr. Dyke prepared himself, and he would say that neither himself nor his family ever felt any injurious effect from its use. Further than that, he would call evidence to show that of the immense amount of "Excelsior" baking powder, which was manufactured at Doncaster, and also sent out from Norwich, not a single patient coming under the care of the professional witnesses that would be called, had ever suffered from dyspepsia or other ailments from its use. The only difference between the alum baking powders and that made from tartaric acid was that the former had a slower action, and many persons consequently preferred it. Having referred to the process of digestion of food in the body, remarking that it was the ptyalin of the saliva that was the principal agent in this respect, the learned counsel observed that it was a remarkable fact that though the Cambridge decision was given eleven years ago, yet there had only been one similar prosecution, that at Ilston, where there was also no conviction, since that time, and assigned as the cause that nobody thought fit to question that decision. The reason why he had been so particular as to mixture and chemical combination was because alum was formerly used in bread, not for the purposes of a baking powder, but in order to make inferior flour look white, and it was a vulgar belief that because an element was harmful, a chemical combination was also harmful. Inspector Jones had been labouring under that vulgar belief,

and he was sorry to say the public analyst (Dr. Morgan) had also confirmed that when he said on the certificate, "In my opinion the use of alum in bread is prejudicial to health." It was the metal aluminium—a double salt—in combination with water that was formed, but that was a totally different thing, and, as he would show, was injurious. Learned counsel also denied that hydrate of alumina was soluble in the stomach, and said that he would call in support of his contentions Dr. W. Richardson, who was known throughout the world; Dr. Luff, Mr. Wynter Blyth, Dr. Dyke, Mr. Sutton, and many other eminent medical men, many of whom had written largely upon questions affecting their profession. In conclusion, he said he could say without hesitation that the theory he should place in opposition to the theory of the prosecution would be quite as strongly supported, and if the learned Bench did not believe the case for the appellant exactly preponderated, yet they must give him the benefit of the doubt.

Mr. Francis Sutton, Fellow of the Institute of Chemistry and Public Analyst to the County of Norfolk and the town of Great Yarmouth, was then examined by Mr. Blofield. He and his family, he said, had used the "Excelsior" baking powder for upwards of thirty years without any ill-effects. He did not believe hydrate of alumina was soluble in the gastric juice, but passed through the intestines undissolved. With regard to Dr. Morgan's experiments, he (witness) did not consider them fair, as he took freshly precipitated hydrate of alumina, which was much more soluble than when it was baked in bread.

Replying to the Vice-Chairman, Dr. Morgan said he did not actually mix the hydrate with the bread, but wrapped it in a muslin bag and introduced it into the dough, after which it was baked as near as possible at the same temperature as other bread would have been.

The Court then adjourned till Friday.

FRIDAY.

The cross-examination of Mr. Francis Sutton, County Analyst of Norfolk and Borough Analyst of Great Yarmouth, was continued by Mr. David Lewis. On Thursday, it was stated by witness that an experiment had been performed upon a groom of Dr. Latimer. It was as follows:—The coachman was given a pound of bread made with the baking powder in question, with the proportions of bread to flour given upon the wrappers of each packet. He commenced to eat it at 6.50, and finished at about 7.15, and to assist him he took three glasses of water, or a pint and a half in all. He then went out for a walk and returned about 9.30 o'clock, when, in the presence of Mr. Sutton, Dr. Luff, and Mr. Wynter Blyth and others, the tube was put in his stomach with a view of getting its contents up through it. Before, however, this could be done, he vomited about a pint of the contents of his stomach. This quantity was put into a dialyser, and left for the night in a room, which was locked, the key being taken by Mr. Sutton. On the following morning the dialysate was examined, and Mr. Sutton alleged that it did not contain chloride of aluminium, as it should have done if the theory of the prosecution was true. Mr. Sutton now maintained that this experiment was a conclusive answer to the experiment by Dr. Morgan upon himself and his son, and further stated that it was carried on under precisely the same conditions. However, upon being further asked by Mr. Lewis, witness stated that the dialyser used in his case was the bladder of a freshly killed sheep, and not, as was the case in Dr. Morgan's experiment, one made of vegetable parchment. Witness also admitted that if there had been chloride of aluminium in the contents of the stomach, it would probably have combined with the albuminoids present in the bladder; whereas in vegetable parchment there would be nothing to combine with chloride. Replying to further questions, he said he was of opinion that hydrate of alumina was insoluble when eaten with bread. About 20 years ago he was consulted by a manufacturer of an alum baking powder as to whether it was injurious. That powder contained about 35 per cent. alum, and after adopting its use in his family and upon himself, as well as upon a dog, he gave a testimonial to the effect that it was harmless. He knew that Borwick's baking powder had obtained many medals at exhibitions, and that the "Excelsior" had not any, but he had no interest in either. Witness also stated, with regard to the albumen contained in the sheep's bladder combining with the chloride of aluminium, he and Dr. Luff and Mr. Blyth had that morning tested the bladder itself, and found there was no chloride present. He admitted that at home he himself generally used a vegetable parchment dialyser, but thought a bladder was fit for the purpose, and, in answer to the Bench, said he was satisfied with the experiment until Mr. Lewis had, in cross-examination, pointed out the difference. Being cross-examined as to whether carbonate of soda by itself would serve the purpose of baking powder by giving off much carbonic acid gas under heat when mixed in flour, he would not

admit that that was so, but said that if it were added to flour and the whole mixed with sour milk, carbonic acid gas would be given off, and it would then act as baking powder.

Dr. Arthur Pearson Luff, Bachelor of Science of the University of London, Member of the Royal College of Physicians, Fellow of the Institute of Chemistry, Physician and Lecturer at St. Mary's Hospital, London, Official Analyst to the Home Office, &c., was next called. He stated that alum and alumina were absolutely dissimilar substances in this way; alum was a soluble body, hydrate of alumina was insoluble; alum was astringent, hydrate of alumina was quite non-astringent; alum contained aluminium as a sulphate, while hydrate of alumina did not, but was an altogether different substance. As a specialist on poisons, he might say that alum by itself and in certain quantities was a somewhat injurious substance, but hydrate of alumina was an inert substance and entirely harmless. He was also of opinion that this substance was absolutely insoluble in the acids of the gastric juice.

The Vice-Chairman: Can you conceive gastric juice operating upon bread containing this baking powder in such a manner as to render the hydrate of alumina soluble?

Dr. Luff replied that he could, in the case of the gastric juice of a dog, because of its strength, but not in the case of a human being.

The Chairman: What is the inducement to use alum baking powders?

Dr. Luff: I should say there are two reasons, one is that alum is so much slower in its generating action than tartaric acid, which means a good deal to a poor woman who has not always got her oven ready. The second reason is that, weight for weight, the alum baking powder is a cheaper powder.

In adversely criticising Dr. Morgan's experiment, the witness first paid a high tribute of praise to the Swansea Public Analyst as a skilful practitioner.

The Chairman: I am glad to hear you make that preface, as we also have a very high opinion of Dr. Morgan.

Dr. Luff, continuing, said he regarded the experiment as utterly valueless, as Dr. Morgan did not use hydrate of alumina as it existed in cakes or bread made with the alum contained in the baking powder in question. He simply took freshly precipitated hydrate of alumina which was not diffused in the flour and baked in the bread, and that made all the difference. With regard to inserting the substance in a muslin bag and then baking it in the centre of a loaf, that also rendered the experiment utterly valueless, inasmuch as the temperature of the interior of the loaf was not the same as on the outside for instance. It was put in the coolest part of the loaf, and consequently, not being distributed throughout, it was not a comparative experiment that could be taken the same as hydrate of alumina baked in bread. Further, Dr. Morgan gave from 15 to 30 grains of hydrate of alumina, and that would represent a quantity, in baking powder, which would be contained in about six pounds of bread. Nobody could eat that amount at once, and therefore the experiment was not a fair nor a comparative one. Would it be fair, he would ask, for him to give a man from 15 to 30 grains of arsenic, and assume that the action would be the same as though a smaller dose were given? They often gave small doses of arsenic, but they all knew that if a larger quantity were taken it was very poisonous. The witness then detailed experiments he had made with a view to ascertaining whether hydrate of alumina was soluble or not. He first prepared hydrate of alumina, and then digested it in test tubes with a solution of .02 per cent. of hydrochloric acid, as representing, as near as possible, the percentage of acid contained in the human gastric juice. In none of his experiments had chloride of aluminium been formed, but, on the contrary, the hydrate of alumina was insoluble. Witness next repeated Mr. Sutton's evidence as to an experiment that had been tried upon Dr. Latimer's coachman in the presence of the witness and the other leading witnesses for the appellant. In this the coachman ate a pound of bread made with the baking powder in question. The contents of the stomach were afterwards digested and dialysed in the same way as Dr. Morgan's, but no chloride of aluminium was found.

Witness also said that his opinion that hydrate of alumina, baked in bread according to the directions on the wrapper of these powders, was insoluble in the gastric juice of the human body, was based upon the experiment made by Mr. Sutton, already detailed by him, upon the experiment made by himself about three weeks ago, and lastly, the experiment upon Dr. Latimer's coachman. The experiment made by himself three weeks ago was as follows:—He took some alum and powdered it in a mortar, and then added some hot distilled water, and rubbed them together until the alum dissolved. He filtered the solution through the best Swedish filtering paper, and warmed the mixture up to a temperature which he could only give approximately—from 150 to 200 degrees Fah. He did not boil the mixture. He then precipitated the hydrate of alumina and collected it on a Swedish filter, and washed it with distilled water heated to 212° Fah. He washed it for three hours, until there was nothing soluble

left. He ascertained that all the soluble substances were washed away. He next removed the hydrate of alumina from the filter-paper, and placed it in a platinum dish, and then put it in a drying oven at a temperature of 150° Centigrade. The bath used was entirely of copper. He baked the hydrate at 150° Centigrade, *i.e.*, 302° Fah. for three-quarters of an hour. He then took different quantities of this hydrate of alumina and digested them in test tubes. He did not analyse the compound before doing so. He knew the compound as dried hydrate of alumina. He could not say what its formula was. He thought it was $\text{Al}_2(\text{HO})_6$; or the same minus a little of the water. A little of the water in combination might have been removed. It might have been oxy-hydrate, which was a different body from hydrate of alumina. He digested this in different test tubes, putting in each tube about five grains, with diluted hydrochloric acid of the strength of '02 per cent., which he believed to exist in gastric juice, at the temperature of the human body in health, *viz.*, 98.5° Fah. There were between 35 and 40 cubic centimetres. The different tubes were digested for different periods, for one hour up to eight hours. He had a dozen tubes and tested the contents of eight. In the first of these digesting had gone on for an hour, in the second for two hours, in the third for three, and so on, each for a hour longer than the others up to the eighth, in which digesting went on for eight hours. At the end of each hour he removed one tube, filtered its contents, and tested the filtrate by the addition of a solution of ammonia, to see if any alumina or hydrate of alumina had been dissolved. No alumina compound had been dissolved. He tested in cold and in heat in every case, and used both tests—concentration and the other. The experiment was done on two days—Tuesday and Wednesday—in the same week. He believed that a '02 per cent. hydrochloric acid was the strength of this acid in human gastric juice. He relied upon Halliburton, the most recent authority, for this, and upon nothing else.

In further cross-examination, he admitted that the only authority for this weak acidity was a table quoted by Halliburton from experiments by Schmidt in 1855, and that Halliburton, while quoting the results, actually pointed out that the acidity was below the normal, presumably owing to the fact that the juice had been obtained from a person suffering from gastric fistula. Also that Halliburton, like other physiologists, accepted 0.2 per cent. as the strength to be used in making artificial gastric juice.

Witness was further cross-examined as to the kind of dialyser that was used, but he was of opinion that the sheep's bladder which they used was equal to the parchment used by Dr. Morgan.

Re-examined by Mr. Brynmor Jones: When alum was introduced into dough according to the directions given on the packets of "Excelsior" baking powder, it was altered by the heat when baked, and no alum was left at all, only hydrate of alumina, which was inert and insoluble in the stomach, and therefore not injurious.

Dr. Benjamin Ward Richardson was next called. Mr. Arthur Lewis was about to read out a lengthy list of the witness's qualifications, when the Vice-Chairman asked if it was necessary. Wouldn't it be sufficient to say the "great Dr. Richardson?" (Laughter.)

Mr. Arthur Lewis (to the witness): You are the great Dr. Richardson?

Dr. Richardson, laughingly: I don't say that.

Replying to questions put him by the learned counsel, Dr. Richardson said he had been 43 years in practice as a physician in London, and amongst other things had specially concerned himself with food analyses, including bread and its manufacture. The reasons why alum was first used were to stop excessive fermentation, and also, probably, to whiten bread. It was common information that there was a great difference between alum and hydrate of alumina. He had had samples of the baking powder in question, and had adopted its use in the bread he had eaten for the past three weeks; but he had not suffered the slightest inconvenience from its use, nor found any trace of aluminium in his constitution. He was of opinion that the use of the alum as contained in the "Excelsior" baking powder was not at all injurious to health.

By Mr. Arthur Lewis: You have heard it suggested in the course of the evidence for the respondent, that chloride of aluminium is formed in the stomach from the hydrate of alumina introduced there?

Dr. Richardson: I don't think that proved, but I would give way to experiment if satisfactory.

Mr. Arthur Lewis: Supposing that chloride of aluminium be formed in the way suggested, in your opinion would the chloride of aluminium so produced by the eating of bread made with this powder be injurious to health?

Dr. Richardson: I don't think it would get into the circulation at all if it were formed.

Mr. Arthur Lewis: And, therefore, would it be injurious to health or not?

Dr. Richardson: It would have nothing more than a local effect, experienced at the time the bread was digested.

Continuing, Dr. Richardson said he did not know of a single case where a patient suffered from any ailment from the use of alum baking powders.

By Mr. Arthur Lewis: What do you say, in regard to the suggestion of Dr. Lauder Brunton, that bulbar paralysis may result from the hydrate of alumina?

Dr. Richardson: I know nothing about it at all, and I rather regret that my learned friend should have made such a suggestion. It is a venture—a new suggestion—with no basis whatever.

Cross-examined by Mr. David Lewis: He had not had time to make any experiment. He heard Professor Dunstan say he had found chloride of aluminium. He would not say he disagreed with him. It was just possible that in some cases there might be a gastric juice of more than usual acidity, which might cause chloride of aluminium to be formed. It was his impression that it had been stated there by the other side that chloride of aluminium was absorbed into the system. He could not controvert that hydrate of alumina was soluble in sodium carbonate. The treatises mostly considered .2 per cent. as the strength of the gastric juice. As to .02 per cent., he was afraid to make any statement. As to .2 per cent., he would rather not answer. It would be brought against him again. He had made experiments, but they were not satisfactory. They wanted a series of experiments to prove that, as it was just possible that that particular gastric juice might have been more than usually acid, in which case there might have been a temporary formation of chloride of aluminium. He did not think that this substance could possibly be absorbed into the system in consequence of the coagulation of the blood. Since he had been in Swansea he had had a sheep killed for the purpose of verifying that point. He found that chloride of aluminium instantly coagulated the blood. He had made special enquiry into the question of baking powders containing alum, and he had come very clearly to the conclusion that from alumina hydrate there was sometimes caused constipation and indigestion. He admitted that, but it never went further than a mere local effect. It never had a constitutional effect. Before giving place to the next witness, Dr. Richardson asked to be allowed to make a statement on the matter.

The Chairman consented.

Dr. Richardson then went on to say that he had controverted some of the theoretical views of his colleagues, and would yield all to them, but he would say that where there was not a single instance of injuriousness, they could not fairly charge a man with a criminal offence who sells—

The Chairman: We cannot allow you to make any observation like that.

Dr. Richardson: Then I put that aside; but when a substance which is taken with impunity by hundreds and thousands of people, and yet has never resulted in an inquest—

The Chairman: I am afraid you are assuming the position of advocate. (Laughter.)

Dr. Richardson: And a substance that has never given a *post-mortem* examination, I say it is impossible that it can be injurious to the system, and it is like straining at a gnat and swallowing a camel to say so. (Laughter.)

Mr. David Lewis: It is not like alcohol. (Renewed laughter.)

Dr. Richardson, who is the well-known temperance physician, replied, amid laughter, "No, it is not."

John Hughes, coachman to Dr. Latimer, was then called, and deposed to undergoing the experiment already referred to.

The Court then adjourned till Saturday.

SATURDAY.

The whole of the following witnesses deposed to having used the "Excelsior" baking powder in their homes without the least inconvenience or apparent injury to health:—Mrs. Martha Johnson, Port Tennant; Dr. Leekey, Mrs. Kingrate, Mrs. Charlton, Mrs. James, all of Pontypridd; Mrs. Williams, Rhondda; Mrs. Phillips, Treherbert, and Mrs. Agnes Thomas and Mrs. Elizabeth Jones, Blaenrondda.

Dr. Dyke, medical officer of health for Merthyr, was next called, and deposed to having used a preparation very similar to the baking powder in question in his family for a great many years without the least harm resulting.

Cross-examined by Mr. David Lewis: It had only been used in pastry and not in bread.

Mr. David Lewis: I don't know whether you suffer from dyspepsia?

Dr. Dyke: Oh, I'm a martyr to it. I have had the cholera four times. (Loud laughter.)

Re-examined by Mr. Arthur Lewis: He did not attribute the dyspepsia to the use of the baking powder.

Dr. Warburton, surgeon to the Bute Engineering Company, and residing at Treherbert, also deposed to having used some of the baking powder in his family without any injurious effects.

Dr. William Taylor, senior physician to the Glamorganshire Infirmary at Cardiff, and Dr. Vachell, Cardiff, late physician to the Glamorganshire and Monmouthshire Infirmary, both gave similar evidence.

In cross-examination, Dr. Vachell said he was exceedingly pained and surprised that a man of eminence like Dr. Lauder Brunton should have lent himself to guess at the suggestion that bulbar paralysis and other obscure diseases were probably due to the absorption of alumina into the system. Witness admitted that he had not himself made any experiments. He relied upon what he read in the medical journals. The *British Medical Journal* was his Bible. (Laughter.)

Re-examined by Mr. Arthur Lewis: The *British Medical Journal* had published a leading article to the effect that alum baking powders were injurious to health, but in the following week it retracted the statement.

Drs. Latimer and Arthur Davies, both of Swansea, were also called, and bore out the previous evidence as to the non-injuriousness of alum baking powders.

The Court again adjourned till Monday.

MONDAY.

On resuming on Monday morning, Mr. Wynter Blyth was at once placed in the box. Besides holding many qualifications and being the author of numerous works relating to hygiene, he stated that he was medical officer of health and borough analyst of Marylebone, public analyst for the county of Devon, as well as a barrister. In his opinion the baking powder in question, used according to the directions, was not injurious to health. He had for the past twelve months used an alum baking powder similar in constitution to the "Excelsior" baking powder, and had not suffered from any ill effect, nor had any of his family applied to him in consequence of any ailment. He agreed with the previous witnesses that the quantity of hydrate of alumina used in Dr. Morgan's experiment was too great, and also that it being freshly precipitated was not the same as hydrate of alumina baked in bread. Mr. Blyth was cross-examined at considerable length.

This concluded the evidence, and Mr. Brynmor Jones, by permission, again addressed the Court. He said he had distinctly challenged the other side to produce a single instance where the use of this baking powder, either in bread, cakes, or pastry, had been attended with the slightest indigestion or injury, but they had not done so, and, therefore, how could it be said to be injurious to health? Besides, it had been proved that about thirty tons per week of alum baking powder came out from Norwich, and surely that fact alone was a strong argument in favour of the appellant. He would remind the Bench that the words "any substance injurious to health" as contained in the Act were very vague. In construing the Act, he submitted, they could not take those words in the broadest possible sense, or else alcohol and divers other substances which, if mixed up with food, would come within the meaning of the Act.

The Chairman: Do you not think the Legislature deliberately intended to use the widest terms that could be inserted in the Act of Parliament in order to protect the public from any kind of harmful admixture.

Mr. Brynmor Jones replied that he thought the Legislature studiously avoided using medical terms, and contended that the simpler words used did not, therefore, imply that they should be taken in the widest sense. The learned counsel then refuted the theory of the respondent that chloride of aluminium was formed from the hydrate of alumina, or that any injury was sustained from the use of alum baking powders, and went on to refer to the experiments as to the solubility or insolubility of alumina hydrate in the gastric juice made on either side. Supposing they put experiment against experiment, what, he asked, became of the proposition advanced by his learned friend? The only point which he thought any reasonable man could take exception to with regard to the experiment upon Dr. Latimer's coachman was the different dialyser that was used than with Dr. Morgan's experiments. In answer to that, one or two chemical experts had said that the sheep's bladder made a better dialyser because it was more like the natural membrane of the body.

The Vice-Chairman: We cannot accept that, as Mr. Sutton admitted that he tried to get a similar one to Dr. Morgan's, and the only reason he used the sheep's bladder was because he failed to get the other.

Mr. Jones : However, I claim on our experiment the verdict of the Court. Continuing, the learned counsel said that the proposition that chloride of aluminium was injurious was based on the scantiest and flimsiest evidence. Who said it was injurious? Dr. Lauder Brunton. But if they put him forward, he would put forward the evidence of Dr. Richardson and Dr. Luff, who both said it was not. It would not be necessary to go minutely into the question of how far Dr. Brunton's arguments and reasons might be well-founded, because they were admittedly problematical. He (counsel) would beg the Bench to consider his general observations, and asked for a verdict at their hands.

Mr. David Lewis followed with a speech of considerable length, and at the outset dwelt upon one or two points of law as to what was meant by the words "food" and "mixed," contained in the Act, pointing out that, according to the evidence, carbonate of soda had been known to be used as a baking powder both in Scotland and North Wales, while in the "Excelsior" baking powder it had been "mixed" with alum. In the course of his remarks, Mr. Lewis said (1) that the other side had nearly entirely misapprehended the case for the prosecution. Mr. Brynmor Jones had stated that the prosecution contended, amongst other things, that when chloride of aluminium was formed it was absorbed into the system. This they had never said. What they did say was that the chloride formed in the stomach did two-fold injury there by irritating the mucous membrane, and interfering with the digesting of fibrin, that when the food got into the large intestine where the fluids are alkaline, the chloride became re-converted into soluble hydrate of alumina and soluble phosphate of alumina, and that a soluble aluminium salt was absorbed into the system. The experiments of Dr. Morgan and Dr. Griffiths were absolutely conclusive, not only in their opinion, but in the opinion of Professor Dunstan and Dr. Brunton, that chloride of aluminium was formed in the stomach when bread made of this baking powder was eaten. Professor Dunstan had, by his four experiments, proved the same thing, and further proved what had never been controverted, that the chloride was re-converted into soluble hydrate in the large intestine—and that in itself was bound to cause the conviction to be upheld, and to establish the case of the respondent—had been proved by experiments, and his evidence upon this point had never been controverted by any single witness for the defence. In fact, no witness had been called to attempt to controvert the statement that the digestion of starchy substances was interfered with to the extent of 27 per cent. Dr. Brunton, whom Dr. Luff described as a man of whom all medical men in England might justly be proud, had stated that whereas it was impossible to say with certainty what was the injurious effect of the absorption of aluminium salts, there was no doubt that such injury would occur, and that possibly some one of the obscure diseases, with whose origin medical science was at present unacquainted, might be due to this very cause. He then dwelt exhaustively upon the evidence, and also concluded by observing that the respondent was entitled to the verdict of the Court.

THE DECISION.

The Bench retired to consider their decision, and at six o'clock, or after about twenty minutes' absence, they returned into Court, when the learned Chairman said : "The Court finds that the appellant unlawfully sold, on December 10th, 1892, a baking powder, which is an article used for food within the meaning of the Food and Drugs' Act, mixed with a certain ingredient, to wit, alum, which is injurious to health. The conviction is therefore confirmed and the appeal dismissed with costs."

Mr. Brynmor Jones : Nothing has been said, sir, about costs. You have given against us as I apprehend, but I will only point out to you that a certain case was made by the prosecution at Pontypridd, and the appeal was founded upon that case. It is now common ground between us, as my learned friend has admitted it, that the case which they have made at these Sessions upon this appeal is a very different one. We brought our appeal upon the basis that we should have to meet the evidence given at Pontypridd, but the whole theory advanced by Dr. Lauder Brunton and Professor Dunstan was novel to us. Having regard to that fact, the appeal should be dismissed with costs.

The Chairman : Your client, if he had just paid his 40s. and costs, would have put an end to the matter, instead of incurring the enormous costs of this appeal.

Mr. Jones : I think the sellers of this article have behaved very properly in coming to the rescue of Mr. James. The whole trade is involved. However, I accept your decision, but I thought it my duty to mention it. Another point I wish to take, and this has been practically agreed between my learned friend and myself, subject to your sanction, that a case should be granted upon the following points :—(1.) That there is no evidence that any article mixed with any ingredient or material injurious to health, was sold, and that baking powder is not an

article of food within the meaning of the Food and Drugs Act, 1875, section 3. (2.) If baking powder is an article of food, then there was no evidence to show that any ingredient injurious to health was mixed. (3.) That there was no evidence that the baking powder actually sold was injurious within the meaning of section 3.

The Vice-Chairman: The last point we cannot agree to. It is a question of fact.

Mr. Brynmor Jones replied that he submitted it as a point of law, and pointed out that no evidence of any specific injury done had been adduced.

The Vice-Chairman: But if we find it was injurious, there must have been evidence that it must injure to some extent.

Mr. Brynmor Jones: But there never has been!

The Vice-Chairman: But we find it.

Mr. Brynmor Jones then withdrew the third point, and leave to state a case was granted on the remaining two points.

REVIEW.

PUBLIC HEALTH LABORATORY WORK. By HENRY R. KENWOOD, M.B., D.P.H., F.C.S., Instructor in the Hygienic Laboratory, University College, and Assistant to Professor Corfield in the Public Health Department; Late Assistant Examiner in Hygiene in the Science and Art Department, South Kensington, &c.; and RUBEY BOYCE, M.B. (London, H. K. LEWIS, 136 Gower Street, W.C.)

The reviewer rises with a feeling of sadness at the thought that in these times of higher chemical education a book like the one under review can be written or sold. The greater part of it, that to which the following lines apply, is intended to teach and guide those seeking Public Health Degrees. It is written by a medical man who possesses the D.P.H. of Cambridge, and who teaches at an important Metropolitan College. Why cannot medical men teach that which they have specially studied and what they know, instead of attempting to enter into matters of which, as a rule, they have but a smattering of knowledge? What if a chemist were to write on anatomy or on pathology? There have been, and there are, a few brilliant exceptions among medical men, who have mastered one or more departments of chemical knowledge; but the writer of 424 of these 491 pages, is by his own showing, not among these. If he were guilty only of retailing second-hand knowledge correctly, of prolixity, and of vagueness, he might be passed over in silence: but the book before us contains so many grave errors, so many mischievous assertions and faulty statements, that it becomes a duty to expose them and to put the student upon his guard.

Lest we should appear to be unjust to the author, we propose to quote from his work somewhat fully, taking first the chapters which are of special interest to the Public Analyst.

Chalk is stated to be frequently added to milk as an adulterant (p. 283). A fair limit for fat in a pure sample of milk is given at 2.5 per cent. (p. 285), and it is asserted that the matter of a fair limit remains unsettled, but must be so fixed as to include milks which contain even less than 2 per cent. of fat (p. 284). The test for boric acid in milk

given is an almost useless one (flame-coloration). The method of fat extraction as used by Dr. J. Bell is declared to be capable of giving "finer" estimations than those obtained by the use of especially devised fat-extraction apparatus (p. 277). We are informed, in mystic language, that the reduction of fatty and non-fatty solids by the addition of water will afford some clue to any watering, "more especially, however, would this be the case with the fatty solids, since these are not fraudulently added to anything like the same extent as the non-fatty solids."

Butter is stated to be adulterated with *linseed and palm oil* and butter-substitutes must be labelled margarine under the *Sale of Food and Drugs Act*. For the estimation of the saponification equivalent, 25 c.c. of "an" alcoholic solution of potash of undefined strength are to be used. Pure butter "never" yields less acidity for 2.5 grammes of the fat than corresponds to 12 c.c. of decinormal soda solution in the Reichert process, and a correction of 2 c.c. may be necessary, in the author's hands.

Fusel oil is stated to be the chief adulterant of spirits (p. 370), and the *ash* of vinegar, *if acid*, is said to show the addition of mineral acid to the vinegar. Sand is still added to sugar (p. 377). Tin in preserved provisions is best tested for by evaporating the solution suspected of containing tin to a small bulk, adding a drop or two of hydrochloric acid, followed by a few drops of a solution of sulphuretted hydrogen, when, if the metal be present, a yellow or blackish-grey precipitate is formed. These precipitates dissolve in "liquor potassæ," but the best corroborative test "is the expensive one of adding gold chloride to the solution acidified by hydrochloric acid and obtaining the beautiful and characteristic colour known as the purple of Cassius." If a student added to, say, the liquor taken from a tin of preserved fruit, a few drops of hydrochloric acid and the same of sulphuretted hydrogen solution and told us that he could detect tin by the reactions given by our author, even if he used the "expensive" reagent alluded to, we should be inclined to think he was "swerving from the truth."

Aniline colours, we are told, if free from arsenic, are harmless. Has the writer of the book never heard of Naphthol-green, Dinitrocresol, Martius-yellow, or other poisonous so-called aniline colours?

The author devotes a large portion of his book to water analysis, and teaches many novelties and processes deserving careful investigation before they are adopted. Lime is estimated by "creating the white precipitate of the oxalate of calcium" by adding oxalate of ammonium, collecting the precipitate, burning it until "the filter paper has been entirely destroyed, the residue being CaC_2O_4 , or the precipitate may be weighed as carbonate"! How this interesting salt, burnt oxalate, may be converted into carbonate we are not informed. No chloride or other salt of ammonium beside the oxalate is directed to be added to the water before precipitation, but the magnesia is nevertheless to be determined in the filtrate, no instruction being given how to treat the ammonium-magnesium phosphate. The silica in water is determined by evaporating 500 c.c. to a solid residue (no acid added

to the water), the residue being "treated with strong hydrochloric acid and then well washed with boiling water." Phosphates are tested for by adding "a few drops of pure and colourless nitric acid and then about twice as much of a solution of the molybdate of ammonium to *some of the water* in a test tube, boiling well." The author expects to get a yellow precipitate under these conditions, and applies the terms recommended by the Water Committee of the Society of Public Analysts, "traces," "heavy traces," and "very heavy traces" to indications alleged to be obtained under his new method of testing.

For arsenic in water, Professor Kenwood employs the Reinsch test (upon the original water), and elsewhere he gives the original Marsh test (deposition of metallic arsenic from the flame upon a cold piece of china) as a sensitive test, and makes no mention of the, to him, unnecessary refinement introduced by Berzelius. For the determination of the albuminoid ammonia he boils the alkaline permanganate, without making it clear whether he boils the whole litre of alkaline permanganate solution for the preparation of which he gives directions, or only the quantity needed for one determination. In either case we should expect his albuminoid ammonia to be excessive. Professor Kenwood does not appear to understand the oxygen process; he still makes his standard-soap solution for hardness estimations by dissolving ten grammes of Castile soap to one litre; he precipitates magnesia, in presence of ammonium salts, by means of "liquor potassæ" (p. 158); and he confounds nitrates and nitrites (p. 77), both as to testing and in stating that nitrites are largely used as a manure.

We are told that "alum" exists normally in the pure flour, and on four other occasions he uses the term "alum" instead of alumina. This series of errors is corrected in the errata, but we cannot avoid the conclusion that the author, when writing, did not know the distinction between alum and alumina, and that the correction is an after-thought.

To give a full account of the errors contained in this book would require almost a reprint of the work in THE ANALYST; a misstatement is to be found on nearly every page. The few quotations we have given must suffice, but Professor Kenwood's book affords a startling illustration of the perfunctory and inaccurate work which passes muster at at least one University, and of the utter worthlessness of the qualification of D.P.H. as a proof of competency to analyse food or water.

We must condole with Prof. Boyce upon the misfortune to have had his contribution on bacteriological research, which is most clearly written and meritorious, included in this work.

The print of the book is clear, and some of the figures very good, notably a picture of a Winchester quart bottle and a sample hamper.

O. H.